

Buchanan (C. M.)

"THE
SUCCESSFUL
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OF
TYPHOID
FEVER."

A REPLY TO DR. PAGE.

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"The Successful Treatment of Typhoid Fever."

A REPLY TO DR. PAGE.

An article with the above-quoted title, and written by Dr. Charles E. Page, of Boston, appeared in a recent number of *The Arena*. It is unfortunate in many respects. In the first place, it is a discussion of a medical subject in a lay journal, where, for obvious reasons, its merits or demerits can not be closely estimated. In the second place, appearing in a magazine of the nature of *The Arena*, and being so well written, it is apt, by its apparent plausibility, to mislead the average reader to whom it is presented. Even well educated and intelligent members of the laity are not in proper position for judgment upon a matter so essentially technical, and requiring special and peculiar knowledge for a complete understanding of the relative value of the points presented. Hence, as a matter of course, such are apt to accept the *ipse dixit* of any one who may possess such desirable faculties. And in such a discussion as this, when the original article has been put in such a plausible and graceful manner, the layman, lacking special knowledge, is prone to base his judgment upon purely extraneous grounds. Therefore, I have no doubt, the article in question has been very convincing to a large majority of its readers. In the third place, several extravagant declarations are made which, though undoubtedly the fruit of ardent enthusiasm, are decidedly untenable in the light of modern bacteriological and pathological research. In the fourth place, the writer, by his very article, directly upholds a rational expectant method of treatment which he apparently so bitterly assails. In the fifth place, the writer makes the mistake of calling or considering one symptom, pyrexia, the whole disease. Had he entitled his article "The Successful Treatment of *Pyrexia* in Typhoid Fever," and eliminated a few scientific inaccuracies, he would have had rather a readable article.

Now, typhoid fever is an acute, self-limited, febrile disease, caused by infection by a specific germ, and is, moreover, characterized by the presence of a con-

stant lesion of Peyer's patches (aggregations of intestinal glands), the mesenteric glands, and of the spleen. It is an acute, self-limited disease—that is, it has no tendency to become chronic, and, also, it runs a certain definite course, when fully established, *regardless of any form of treatment*. This, in a typical case, is usually about four weeks in duration. There is no specific known. The so-called "specific methods," involving the use of calomel, tincture of iodine, carbolic acid, etc., fulfilling absolutely none of the promises so glowingly held forth. Therefore, the function of the physician is necessarily almost passive; he must watch and wait, he can not combat the disease as a whole, he must and can combat dangerous symptoms as they arise; it is solely in this guise that the physician is the preserver of his patient's life. But he must possess skill, knowledge, and decision, to perform even this function.

The chief dangers to the patient are intestinal hemorrhage, intestinal perforation, peritonitis, pyrexia, and, to a certain extent, pneumonia and albuminuria, either one of which may, and frequently does, endanger life, rendering a prognosis grave. Therefore, a neglect to treat the disease symptomatically or expectantly, which is not a matter of choice, might prove undoubtedly a fatal error. The symptomatic or expectant (Latin, *expecto*, I await) method is that method followed where one awaits the advent of grave or threatening symptoms, and combats them as they arrive. It is, of course, as one may readily judge, the only rational method of treatment in all self-limited affections, where no specific treatment is available.

The onset of the disease is insidious, and its prodromata vague and general. After the stage of incubation has passed, and the invasion of the disease becomes obvious, the fever is usually one week in reaching its acme, where, in typical cases, it remains approximately for about two weeks, and is followed by the stage of febrile decline or decrement, which usu-

ally lasts a week. We see here, then, a regular procession of morbid processes over whose *course* science has absolutely no effect, but whose *intensity* it may frequently abate. This, therefore, is the physician's attitude: he may not by one fell grasp, snatch his patient from danger, but he may, and must, stand above him, protecting him and battling the disease single-handed at every step, until by watchful care and wary eye he has saved his precious and defenceless charge. All this means, as we have said before, that the expectant method is the only rational one—it is not a matter of choice, it is forced upon us. This is the consensus of opinion among authorities, even those who favor the method of Brand, which is undoubtedly a valuable factor in a rational, expectant mode of treatment.

The disease is caused by an invasion of the Eberth typhoid bacillus, which is most often accomplished or effected by the ingestion of infected food or drink. This bacillus is short, thick, motile, and has rounded ends, one of which contains a glistening spheroid body believed to be either a spore or an area of dense protoplasm. This specific germ is *constantly* present in typhoid fever, and constantly present in the lymphoid tissues of the intestines (and here invading the glandular structures), in the mesenteric glands, in the spleen and in the liver. It is found also in the intestinal contents and in the feces, but is not present in the excrementitious products of the skin or lungs. Hence, infection of a healthy individual by a diseased one is usually effected, not by personal contact, but through the agency of the alvine discharges. There can be no typhoid fever without the presence of its specific etiological factor—the germ. Therefore, when Dr. Page says of the milk diet, "In the writer's estimation it would be difficult to devise a better prescription if we desired to produce typhoid fever," he simply makes a mistake. We would suggest to him, however, the use of pure cultures of the specific bacillus; he will find them to work like a charm for the above-quoted purpose. Any infected food, that is, food containing typhoid bacilli, is liable to produce the disease, and no food free from such bacilli can, *per se*, cause it. Certainly, while a pure culture of the

germs may not be as palatable as the milk diet, nevertheless it would be decidedly more efficacious than the latter in the production of the disease. Hence, Dr. Page's fling at the milk diet is not only a misconception, but is unsupportable, to say the least, for the milk used in such a treatment is, or should be, sterilized before administration, and no physician of broad training would do otherwise.

Not every one, however, exposed to infection becomes diseased; this is owing to personal condition and idiosyncrasy, just as every one exposed to small-pox does not invariably contract the disease, or just as every vaccination does not "take."

Now, the germ usually enters the system by the alimentary tract, where it finds lodgment, causing functional derangement of the affected parts, and thus alimentation is the first vital process with which the disease interferes. This causes more or less disturbance of nutrition, and to overcome this obstacle is one of the first considerations engaging the mind of the man of medicine. Lacking its natural nutrition, the organism turns and literally feeds upon itself, thus drawing largely upon a stock of vitality already greatly impoverished, and causing most rapid emaciation and debility. So great is this demand that even the most vigorous supporting treatment does not altogether eliminate these disagreeable circumstances. It then becomes expedient, nay compulsory, that we furnish the debilitated system with proper nutriment. As Dr. Page well shows by quotations from Dr. Anton Gluzinski, of Cracow, the alimentary tract is not only not in proper condition for the function of *digestion*, but even the unassailed portions are debilitated, so far as digestion is concerned, by the morbid process going on in the ilio-cæcal region. However, though digestion is impossible, *absorption* is not altogether so. As Dr. Page says, "the entire energies of the animal organism are engaged in the effort to eliminate unassimilated nutritive matters." That is just it; in the words of Bassanio, "I thank thee, Jew!" and so the conclusion is reached by him that we must starve an already starved organism. More arrant homeopathy was never preached by Hahnemann. Does it not occur to every rational physician that

pre-digested foods must be used, then there would be no "unassimilated nutritive matters to eliminate, and absorption, a passive process, would be all required—this, I need not add, is possible even to *devitalized* intestinal membrane. Of course there is a right way and a wrong way to do everything, and perhaps Dr. Page has met some one who has made use of the system he so deprecates, with reason. But his suggestion, though an improvement on the obsolete methods he criticizes, is surely not the best. Dr. Page should be charitable enough to refrain from applying the Sinonian policy of "*Disce omnes ab uno*," which was proven to be decidedly unsafe at its inception. No one man, be he good, bad or indifferent, can represent the modern system of medicine, nor can the obsolete practices of one man bring the profession at large into disrepute, for there are black sheep in all flocks.

Now as to the febrile condition; this, very frequently, in well-marked cases, induces such a state of hyperpyrexia as to immediately endanger life. This *symptom* must, then, be combated—this is not the disease itself, but one of its major symptoms; hence any treatment which has for its object the lowering of the temperature is but part of a system of symptomatic or expectant treatment. And this is exactly and altogether the relation and use of the Brand method, which is never used until the temperature reaches from 101° Fahrenheit to 103° Fahrenheit—at the Johns Hopkins Hospital, when the thermometer indicates a temperature of 102.5°. This febrile condition is due to absorption of the ptomaines and toxalbumins, elaborated or formed in the life-history of the specific bacillus. To combat this symptom one may use antipyrin, antifebrin, phenacetin, quinine or the cold bath, cold pack or method of Brand. Of the three drugs first named, the first two, although they reduce temperature effectually, are objectionable, being pronounced cardiac depressants. Phenacetin is least open to this objection, and is hence the safest of the three, although not quite so efficient as antipyrin. The cold bath and the cold pack are very efficient, but so depressing as to be used by most conservative physicians as a *dernier ressort*. The method of Brand

is undoubtedly *when properly applied*, an excellent means of reducing temperature, that is, of combating this febrile symptom—mark you, not *disease*, but this *symptom* of the disease. Of course hyperpyrexia, being common to most acute affections, is not a pathognomonic symptom of typhoid fever, but merely one of several subjective and objective symptoms occurring in many affections, especially the acute exanthematous diseases. So that the method of Brand is applicable to most acute febrile disorders and a great deal that Dr. Page says of its use in typhoid fever is, with very few exceptions, undoubtedly true.

This method is not, as one might suppose, of recent origin, but was introduced by Currie, of Liverpool, at the end of the last century, and of late years was brought to the attention of the medical profession by Brand, of Stettin. Its use should be enforced at hospitals, but it can not always, though extremely desirable, be rigidly enforced in private practice, for reasons which are obvious.

The following is the method as used in the Johns Hopkins Hospital under Dr. Osler:

"Every third hour, if the temperature is above 102.5°, the patient is placed in a bath (at 70° Fahr.), which is wheeled to the bedside. In this he remains from fifteen to twenty minutes, and is then taken out, wrapped in a dry sheet and covered with a light blanket. Enough water is used to cover the patient's body to the neck. The head is sponged during the bath, and, if there is much torpor, cold water is poured over it from a height of a foot or two. The rectal temperature is taken immediately after the bath, and again three-quarters of an hour later. The patient often complains bitterly when in the bath, and shivering and blueness are almost a constant consequence. *Food is usually given with a stimulant after the bath. The only contra-indications are peritonitis and hemorrhage.*" (The italics are mine.) Dr. Osler ascribes the following good effects to the baths:

1. Reduction of fever.
2. The intellect becomes clearer, the stupor lessens, muscular twitchings disappear.
3. A general tonic action on the heart.
4. Insomnia lessened, patient usually

falling asleep for two or three hours after each bath.

5. Mortality reduced to a minimum.

Of course 2, 3, 4 and 5, to a greater or lesser extent, necessarily follow from 1.

He also says, "This rigid method is not, however, without serious drawbacks, and personally I sympathize with those who designate it as entirely barbarous."

Again, M. Sevestre, of the Society of Hospitals, says that the cold baths are generally useless in the typhoid fever of children. He says that they may be of advantage in grave cases, but are not without danger, and their employment should always be superintended. The heart, in fact, is often affected in the typhoid fever of childhood, and the speaker has frequently observed, after a cold bath, a tendency to syncope, collapse, and especially an enfeeblement of the pulse. These facts have led him to avoid the use of cold baths in this disease occurring in children.

Now, that Dr. Page is an enthusiast, and has painted the picture in glowing colors, we may easily show. Dr. Liebermeister, a noted German authority, says of Brand that "his book 'On the Hydrotherapy of Typhoid Fever,' although in some respects one-sided, evincing the enthusiasm of subjective conviction, rather than objective presentation and analysis of facts, was nevertheless calculated to recall attention to this method." Then, again, Leichtenstern tells us that he has observed the effect of one thousand nine hundred and sixty cold baths in typhoid fever patients, and that in three hundred and eighty of these the temperature of the body was as high two hours after the bath as before, *and sometimes higher*. That is, this happened in slightly more than twenty per cent of the cases.

Again, Ziemssen and Immerman found by their investigations that the degree of the lowering of temperature, as well as the duration of the same, was *much greater in the mild attacks than in the severe ones*. That is, in those cases where the baths were needed most, if at all, they were much less efficacious. Leichtenstern also found the average lowering of temperature to be greater during the fourth week than during the second and third weeks. Now the fever is at its height in the second and third weeks,

and declines naturally in the fourth week; hence Leichtenstern's investigations, clinching those of Ziemssen and Immerman, prove the baths to be effective where there is little or no need of them. Hence, paradoxically expressed, they are most useful when they are useless—that is, as far as saving life is concerned.

Again, Dr. Liebermeister, who wrote the masterly article on "Typhoid Fever" in Ziemssen's classic work, "The Cyclopædia of the Practice of Medicine," says: "The fact is, notwithstanding the high estimate that I place upon the cold water treatment, and my positive conviction that it would be wrong to treat a severe case of typhoid fever without the systematic abstraction of heat (unless there were complications present which forbade it); yet, if I were forced to the unpleasant alternative of adopting only one or the other of these two means—cold water or quinine—I should, in the majority of cases, choose the latter."

Then, again, Dr. Page's statistics are decidedly unfair in the light of more complete investigation. Among the most recent of *large* returns, and hence nearer the truth, are those from the Brisbane Hospital, Australia. They are as follows:

No. of Cases.	Treatment.	Mortality.
1,838.....	Expectant.....	14.8 per cent.
171.....	Incomplete bath.....	12.3 per cent.
797.....	Strict bath.....	7 per cent.

Dr. Page gives the expectant treatment the rather indefinite mortality rate of "more than twenty per cent," while he credits the bath treatment with a mortality of 2.7-4.7 per cent. Thus we see he puts up the former and lowers the latter. The mortality in indifferent expectant treatment is slightly more than twice as great as in the strict bath treatment, while Dr. Page would have us believe it about seven times as great. Then, again, the comparisons made are unfair. In private practice the strict bath treatment can not be always, in fact seldom, used; hence, we have to fall back upon the incomplete bath and the "expectant treatment." The bath treatment can and is enforced in hospitals. Of course the conditions are obviously unfair for comparison. In private practice we can never produce the excellent conditions of hygiene, medical attention,

nursing and dieting which are *enforced* in hospitals; the truth of this statement any physician will uphold. Now note carefully—the system of Brand, a method proven by the investigations of Leichtenstern, Zeimssen and Immerman, to be only of material effect in *mild* cases, or in those in which the disease is *declining*, this method is more universally applied in hospitals where every condition present is one favorable to the patient's recovery. *Every condition is favorable here to a low rate of mortality.* The expectant treatment we are forced to apply to almost all cases treated *at the patient's home*; we have an entire absence of those absolute and essential hygienic conditions found in every well-regulated hospital. All question of treatment aside, the conditions here are unfavorable to a low rate of mortality. But transpose the conditions surrounding the two methods and there will be no very great difference in the mortality rate. I simply quote this to show that statistics place the expectant treatment apparently in a more unfavorable light than it properly deserves. Then, moreover, bare unqualified statistics possess little or no value; they should state the *intensity* of the disease, and the *hygienic* or other conditions surrounding it during treatment. For instance, suppose we should treat six patients, with mild cases of typhoid, expectantly, and have perfect hygienic surroundings; they would probably all recover, or else the rate of mortality would be exceedingly low. Now, on the other hand, suppose we treat six very severe cases, with poor hygienic surroundings, by the method of Brand (which is most efficacious in *mild* cases); they will either all die, or else the rate of mortality will be very high. They would be tabulated thus:

No. of Cases.	Treatment.	Mortality.
6	Expectant	0 per cent.
6	Brand	100 per cent.

The inference will be obvious, *i. e.*, that in the latter case the method of Brand was responsible for the deaths, since none occurred under the expectant treatment. This, of course would be as manifestly unfair to the method of Brand as the almost valueless statistics are now to the expectant method. So, statistics *unqualified* prove nothing.

Again he says, "The hospitals of Phil-

adelphia are generally adopting the German (cold bath) system of treatment of typhoid and typhus fever." This is incorrect, and gives credit to Germany for what she has no claim to. The cold water treatment is not new, it is not German, it was introduced and practiced by Dr. Currie, of Liverpool, as long ago as 1787; he claimed that it had the power not only of moderating the symptoms but frequently of cutting them short. He drew as enthusiastic a picture of its merit as does Dr. Page, and the method enjoyed a high degree of popularity for twenty or thirty years, when it fell into disuse owing to the exaggerated character of the claims made for it by its advocates. It was *revived* by Brand, of Stettin, in 1861; consequently, it is not proper to give Brand the credit, nor to speak of the treatment as the "method of Brand."

Dr. Page says: "Why is it that to-day, throughout the civilized world, the treatment is practiced thoroughly *only* by a few individuals here and there, and half-heartedly by a few others, while, speaking of the profession in general, almost no use whatever is made of it." Over and against this I would like to place the statement of Dr. Hutchinson, who wrote the article "Typhoid Fever," in Pepper's standard "American System of Medicine." Dr. Hutchinson is connected with the Pennsylvania Hospital and Children's Hospital in Philadelphia, and having had a professional experience of more than thirty years, is well qualified to speak with authority. He says of this method of treatment, "the recorded observations of Bartels, Jürgensen, Ziemssen and Liebermeister, in Germany, and of Wilson Fox and others, in England, have so far restored the treatment to professional favor *that there are few physicians, either in this country or abroad, who do not occasionally have recourse to it.*"

While I agree with Dr. Page in what he says of the good effects of the cold bath and compress in pneumonia, nevertheless his statement that a hot mustard poultice to the chest will produce pneumonia, if applied to a typically healthy man, is on a par with the statement that the milk diet will produce typhoid fever. In the early stages of pneumonia there is a congestion of all or a portion of the pulmonic areas; and it is beyond my

powers of comprehension how a measure which can and does cause a peripheral determination of blood and consequent depletion of an engorged central area, to a more or less extent, can either aggravate the morbid pneumonic process or generate such a process in the healthy individual.

Now, if we wish to determine the value and proper status of the "method of Brand," let us not take the account of the enthusiastic admirer nor that of the enthusiastic decrier; but, as in all precise scientific work, let us find the general average, and even then allow a slight margin of error on the basis of "personal equation." We should not cast away all other aid and rely wholly upon a means which Osler characterizes as "barbarous," Leichtenstern as more efficacious in the decline of the disease, Ziemssen and Immerman as more effective in mild cases, Liebermeister as inferior to treatment by quinine, and, by Leichtenstern again, as exceedingly transitory in its effect—this Dr. Page, apparently, would have us do. M. Sée, of France (*Journal de Med. de Paris*, 1888), considers quinine superior as an antithermic to cold bathing, because the former is tonic to the heart, and, also, because it decreases tissue oxidation. This conclusion he bases on experimental research. Nor, on the other hand, should we by any means totally reject a means which has undoubtedly been productive of some good effect. The method is neither a panacea nor an imposture, but one of those good old-fashioned methods which is cast aside and picked up again, apparently as caprice alone wills.

There are fads in all ranks and departments of life—the medical profession is no exception to this general rule. A physician will try a new drug, and under favorable circumstances obtain only very favorable results; he forthwith indites a long enthusiastic letter to some medical journal, in which he recounts all of the virtues and none of the bad qualities of the particular therapeutic agent. A portion of the profession is all agog—a revolution, they say, is about to take place in methods of treatment. Soon an article comes to the self-same journal; another physician has used the same remedy in cases which evidently were not so favor-

able—he decries the drug, and denies its power to do anything. And so the war of words and facts is waged until time and experience have placed the drug where it belongs—neither in heaven nor in hell.

The method of Brand, or rather of Currie, has been in use for more than one hundred years with its various ups and downs, and is beginning to rise again upon the crest of the wave of popular favor. And so, at present, medicine is passing through the stage of the fad of cold-water treatment; revived just as the bustle was revived some years since. This will probably grow until its huge dimensions, like the bustle (*tournure* sounds a little better), will sink it in innocuous desuetude for another period of time. Let us, however, try to steer a middle course—"proving all things and holding fast to that which is good." And so thou "method of Brand," hoary-headed with age, born before Brand himself saw the light, we welcome thee again to our large and increasing fold—welcome thee, because, though not altogether an angel of light, thou art surely not an imp of darkness.

In conclusion, I would like to express the hope that I may not have seemed harsh in strictures, for the advice of Pope has been held constantly in mind—

"In every work regard the writer's ends;
For none can compass more than they intend:
And if the means be just, the conduct true,
Applause, in spite of trivial faults, is due."

Dr. Page writes in a sincere but superabundant strain of enthusiasm, and with a grace of diction not altogether common to medical writers. After the chaff has been blown away, there is certainly something of definite value left. Nevertheless, an article of its style and scope is hardly in place in a lay magazine; probably *The Arena* may take exception to this, and exclaim with Bacon, "All knowledge is my province!" However, a large majority of its readers, though exceedingly intelligent, are non-professional, and, as is well known, the laity is prone to regard every word of a physician as an inspired and dogmatic utterance. Hence this "barbaric yawp," as the good gray poet would say.

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